through some omission, not easily to be explained, if it be not the effect of a mere accident, geographical proficiency has never hitherto been adequately encouraged. Consequently, the Geographical Society has thought it right to step in to supply the needful encouragement. There is another good reason for the interference of the Society, in the fact that facilities of travel have rendered our interests much more cosmopolitan than formerly, while the public schools of the old-established type, have made no corresponding change in their curriculum. Mere youths now-a-days have exhausted the grand tour of two generations back, and a year or two of early manhood is often spent in America, Australia, and India, while books of travel load our library tables. It seems monstrous that a so-called liberal education should not qualify men to journey themselves, or to read the journeys of others, in an intelligent manner.

Mr. Wilson remarks, and his remark deserves respect, that the masters of Rugby were almost unanimous in rejecting the invitation of the Geographical Society, but I can fairly retort that other scholars no less practised in education and no less competent to decide, pronounced our system of prizes to be a valuable and much-needed institution.

It would be easy to write at great length in support of what we have done, and I might perhaps be expected to say something on the respective objects of the political and physical geography prizes, but I do not wish to provoke a discussion in your pages, because I am on the point of going abroad and should be unable to take further part in it. FRANCIS GALTON

"Kinetic" and "Transmutation"

1. WHEN, in 1864, I wrote for the *Reader* the history of the Baconian Philosophy of Heat, I found in use, in connection with the subject, the term "dynamical theory of heat," in English, which was employed as an equivalent for the expression "mechanische Wärmetheorie," current in German. The word "dynamical," already so vague from frequent abuse, corresponded but little, when used in its proper meaning, to the real intent of the theory in question; and the same remark applies, with at least equal force, to the word "mechanisch," even wider in its scope and as often misused. I was thus led to adopt the word "Kinetic," to supersede the above; and that in preference to the current word, "cinematic," which, in conjunction with "theory," would imply a tautology.

I am glad to see that Sir W. Thomson and Professor Tait, in their treatises on Natural Philosophy and on Heat, as well as in some remarkable papers on Atoms which have appeared in NATURE, frequently make use of the same word, "Kinetic," in connection with the theory of heat and of gases, as also in conjunction with "energy." Instead of the expression, "actual energy," originally introduced, I believe, by Mr. Rankine, Sir W. Thomson and Mr. Tait employ the term "Kinetic energy;" and from various motives, linguistic as well as strictly scientific, I venture to think that the original wording of Mr. Rankine in the case of "potential energy," should be likewise superseded, viz., by "dynamic energy."

2. In the *Philosophical Magazine*, I have been rated, indirectly, by Professor Challis, (for no mention is made of my name in connection with the subject), for having applied the word "transmutation" to rays, without recalling the fact of his having done so before me. I considered the expression "transmutation of rays" as the abbreviated and thoroughly English rendering of the words, "change of the refrangibility of rays, or light," used by Professor Stokes; and as such, requiring no authority but the precedent furnished by the existence of the analogous expression of "transmutation of matter." If, however, an authority had to be cited, it would have been Euler, in whose "Nova theoria lucis et colorum" (Opusc. var. argum.) the following passage occurs:—"Cum igitur a corporibus rubris radii tantum rubri, et a violaceis violacei ad nos pertingant, etiamsi radii albi in ea incidissent, manifestum est istam transmutationem a sola reflectione proficisci non posse."

As I have returned to this subject, I may be permitted to express my astonishment that Professor Challis, who thought it due to him that his name should be mentioned for being the author of the expression "transmutation of rays," should have on his part omitted, in speaking of the transmutation of Herschelic rays into Newtonic, a reference to my own share in the *res gestæ*. When I see the same thing being done in so widely circulated a treatise as that of Mr. Brooke on Natural Philosophy, and in one intended for even more popular reading, reproducing the teaching of the Polytechnic, I might think of entering a protest, if experience had not convinced me of its uselessness.

C. K. AKIN

Parturition of the Kangaroo

I BEG leave to call your attention to certain comments in your issue of the 23rd of June on the proceedings of the last meeting of the Royal Geological and Zoölogical Societies of Ireland. It is usual when parenthetical observations are made in any journal without the customary affix "Ed." to ascribe them to the printer's devil. Now, your devil, in commenting on an *imperfect* report of your Dublin correspondent, would lead your readers erroneously to infer that I had adopted the ideas which he has been pleased to call "absolute nonsense," and takes me to task for saying "that the actual passage of the fœtal kangaroo from the uterus to the pouch was not yet proved;" he himself stating that my remarks were "in contradiction to the facts observed by the late Earl of Derby's father or by the present Professor Owen." Now, a critic calling in question the words of others should be careful of his own. No facts on the subject were observed by the late Earl of Derby's father, and Professor Owen, after elaborate arrangements for the observation, states that "as parturition took place in the night, the mode of transmission to the pouch was not observed." (Phil. Trans. for 1834, p. 344.) There have been four observers in this matter especially worthy of being noticed:—(1) the keeper at the Zoological Gardens, Knowsley, who, according to Lord Derby's statement, saw the young kangaroo born, and that it was placed in the pouch by the paws of the mother (Proceedings of Zoological Society for 1833, p. 132); (2) Professor Owen, as referred to above; (3) Mr. E. G. Hill, who, at thirty yards' distance, saw the kangaroo with her mouth take up what he thought was a stone, open the pouch with her paws, and place it in the marsupium, and that he shot the animal and found a newly-born fœtus in the pouch (Proceedings of Zoological Society for 1867, p. 476); (4) M. Jules Verreaux, who is mentioned by M. E. Alix as having seen the kangaroo remove the fœtus from the vulva with her mouth, and place it in the pouch (Annals of Natural History for 1866, p. 316). These all differ as to the actual facts observed, and would seem sufficient to justify me in the statement I had made. That Professor Owen does not consider the question settled, may be inferred from his concluding observations on the subject, "whether the circumstance of the parturition is constant, viz, the dropping on the ground, or whether the fœtus may occasionally be received by the mouth from the vulva, I am disposed to regard as a matter for further observation; but the main fact of the conveyance of the fœtus to the pouch by means of the mouth may now be held as the more probable (at least the more usual, if not the constant) way in the genus Macropus" (Proceedings of Zoological Society for 1866, page 382). I refrain from any comments, but I thought it right to remonstrate against statements which I felt were injurious to me, to the Society to which I have the honour to belong, and to the advancement of science. JOHN BARKER, M.D.

Dublin, July 1

The Extinction of Stars

IF you will kindly permit an amateur to rush in where astronomers fear to tread, I shall be glad to offer a few remarks on the above subject.

The progress of science enables us to trace, with a probability almost amounting to certainty, the career of a star from its birth; from the most diffused condition of its parent nebula; through the stage of primary agglomeration when it shines as our sun; through the process of cooling into a dim and cloudy spheroid, such as Jupiter or our earth; until cold rules supreme, and the once glowing orb rolls on, barren as our moon.

But when we have reached this stage, we have by no means done with the star. It must continue on its course, and, though in obscurity, it must retain its momentum and its attractive force. Our sun will thus one day travel in darkness, attended by a cohort of funereal planets, and perpetual night will reign over the solar system. This result appears to be but a question of time, and we are, therefore, led to the consideration that many systems must, in all probability, be already extinct, and wandering unnoticed. But as extinction is a gradual process, there will be multitudes of stars in various stages of dimness, and the brilliancy of any orb, its "magnitude" in fact, will therefore depend on its age, quite as much as on its size or distance. On this view, Sir W. Herschel's method of "star gauging" cannot be relied on for a correct determination of the actual shape of the cluster called the "Milky Way," as instead of taking the average of brightness only, as an indication of the average of distance, we have to superadd the average of age. Now, the smaller the star, the more quickly will its light die out, and, therefore, the necessary extent of our galaxy is immensely reduced; in other words, it appears that while the space separating us from the nearer stars, for which parallax has been obtained, remains of course unchanged, the computed distances of those hitherto considered to be farthest off, will be much lessened, as there appears to be no reason for concluding that telescopic stars are necessarily more distant than bright ones for which we cannot obtain parallax, but simply that they are older, or smaller, or both, and therefore dimmer.

Mr. Proctor, in "Other Worlds than Ours," argues that as telescopes barely reach the outermost stars of our own cluster, therefore it is impossible that they should reach to and resolve clusters constituting other systems and lying at distances enormously greater, and therefore that the resolvable nebulæ must lie within our galaxy. If my idea that the stars of our cluster which the telescope shows with difficulty, are not distant but dim, be correct, Mr. Proctor's argument appears to lose its force.

It will be readily allowed that if the light of the stars be fading away, a vast number may have already become extinct, and that it is indeed possible that the orbs now visible may be but a small surviving remnant of far greater multitudes which once illumined the heavens. If our cluster then be much reduced in extent, and its constituents be largely increased in number, it would follow, I imagine, that the chances against collision would be much reduced, and it then becomes less difficult to conceive the possibility of such an event having occurred in the case of the recent outbreak in τ Coronæ Borealis, especially if it were caused by the unobserved approach of an extinct body. This outbreak is usually ascribed to a sudden conflagration of hydrogen, the star being, as Prof. Roscoe says, "on fire." But a star self-luminous surely must be always on fire, and if it contain hydrogen, that gas must be in a state of constant conflagration. The temporary brilliancy of the star seems rather such as would be occasioned by a collision with some comparatively small body, whose impact was yet sufficient to generate heat enough to accomplish its own disintegration and ignition. Let us suppose that collisions are possible, and that their frequency is merely a question of the chances. What would be the consequences of such an event? I imagine that they would depend chiefly on the relative momenta of the colliding bodies; that if one were very much larger than the other, and the velocities high, the temperature would be raised sufficiently to dissipate the smaller into gas, while merely heating, or possibly liquefying the larger. If the bodies were nearly of a size, and their momenta were great, possibly both would be reduced to a gaseous condition; in either case their tendency would be to form ultimately a body equal in weight to the sum of its two constituents. Either the larger body would annex the smaller, or, if both became nebulous, the fervid gases would radiate their heat and contract anew into a system possibly containing a sun and planets.

Again, supposing that two bodies approach each other in such a manner as to avoid a collision, that is, so that their mutual gravity causes them to leave their paths and revolve round each other, we should have the explanation of the existence of double, treble, multiple stars; we should also understand how it happens that some stars (Sirius, for instance,) are accompanied by non-luminous orbs. Also, it would seem that if extinct stars are really far more numerous than is generally supposed, the theory which regards the revolution of attendant dark bodies as one cause of the variability of certain stars, receives fresh support.

Thus, in the course of time, nebulæ would form suns, suns would grow cold, or, while yet glowing, would come into contact and combine with other suns, till gradually space would be peopled with suns, larger and larger, but less and less thickly strewn. Pursuing the idea, we arrive at a period when all the stars of each galaxy shall become agglomerated into one mighty globe—nay, when all these vast galactic suns shall come together and form one solitary orb, in which all the matter once scattered through space shall be collected, accomplishing its successive fates as a sun without a system—a world without a sun—a cold and naked ball.

EARDLEY MAITLAND

Why is the Horse Chestnut Tree so called ?

DURING the spring this tree is the ornament and pride of our public and private parks. In "Woodland Gleanings" it is stated to be a native of the north of India, and is supposed to have been introduced into England about 1575.

Our observant forefathers have given it the very significant name of *Horse* Chestnut (*Castanea Caballina*),* to distinguish it from all other species of chestnuts. The *reason* for so doing I have never seen stated in print; but from the three specimens of cuttings from a branch of this tree which I enclose, it will be very manifest. All over its branches, at every bud, can be seen what at a glance will be taken for an exact conformation of the *foot of a horse*, exhibiting the hoof, the nails of the shoeing, the fetlock-joint, &c., in marvellous miniature, some, of course, better developed than others. This curious freak in nature's vegetable kingdom, has, no doubt, been the origin of our nomenclature of this tree; and it would be an interesting point of philological inquiry to ascertain *whether or not its native Asiatic name has incorporated or associated with it that of the horse?*

I write with the view of eliciting information on this point, and with the hope, too, that some of your botanical contributors will throw further light on this peculiarity.

EUGENE A. CONNELL

Fall of an Aerolite

A LETTER of the year 1628, "sent by Mr. John Hoskins, dwelling at Wantage, in Berkshire, to his son-in-law, Mr. Dawson, a gunsmith dwelling in the Minories without Aldgate," and preserved among Nehemiah Wallington's Historical Notices (i. 13) contains the following narration:—

"On Wednesday before Easter, being the ninth of April, about six of the clock, in the afternoon, there was such a noise in the air, and after such a strange manner, as the oldest man alive never heard the like. And it began as followeth:—First, as it were, one piece of ordnance went off alone. Then after that, a little distance, two more, and then they went as thick as ever I heard a volley of shot in all my life; and after that, as if it were the sound of a drum, to the amazement of me, your mother, and a hundred more besides; yet this was not all, but, as it is reported, there fell divers stones, but two is certain, in our knowledge. The one fell at Chalows, half a mile off, and the other at Barking, five miles off. Your mother was at the place where one of them fell knee deep, till it came at the very rock, and when it came at the hard rock it broke, and being weighed, all the pieces together, they weighed six-and-twenty pound. The other that was taken up in the other place weighed half a tod, 14 pound."

I do not know whether there may be any other record of this remarkable aerolite, so simply but graphically described. Is it not just possible that some of the fragments may yet be preserved in the neighbourhood of its fall? At any rate a search would involve but little trouble.

T. W. WEBB

ANDERSON'S UNIVERSITY

WE extract the following from the *Evening Citizen* (Glasgow) of June 22:—

"The annual meeting of the trustees of Anderson's University was held this afternoon within the institution. Mr. William Ewing, in the absence of the president, was called to the chair. In the annual report which was submitted, reference was made to the death of Dr. Penny in November last, and the appointment of a successor. Mr. Young, of Kelly, who had arranged to set aside 10,000 guineas for the endowment of a Chair of Technical Chemistry in connection with the University, had, it was stated, no further proposal to make, he leaving it to the trustees to make what alteration in the deed of trust they may think proper. Under these circumstances, the managers recommended that advertisements be issued for a successor to Dr. Penny; that the chair should in future be styled the Chair of Scientific Chemistry; and that in electing the professor power should be reserved to the trustees to create such other chair or chairs of Chemistry in connection with the University, and elect such additional professor or professors to fill said chairs as the trustees may see fit; and also to arrange and define, from time to time, the respective departments of the subject to which each professor, including the Professor of Scientific Chemistry, should devote himself. Regarding the mode of electing professors, the

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* The scientific name of the horse-chestnut is *Æsculus hippocastanum* it has no relationship to the *Castanea*, or sweet chestnut.—[ED.]